may be got rid of, or at least, so much paled as to betray their character as interlopers. This, however, is an amount of labour hardly to be expected of those who make tubes in a commercial way, and it is to be regretted that in Mr. Capron's painstaking research, he was compelled to employ such tubes instead of preparing them for himself. In a future research we would suggest the employment of tubes thoroughly heated and washed out with air in the first instance, and then worked with a blowpipe fed with pure hydrogen.

Unfortunately throughout, the tubes employed both by Mr. Capron and by Dr. Vogel seem to have been of doubtful purity. That figured on plate xiv. as hydrogen, contains bands of most suspicious resemblance to those of nitrogen, while the oxygen tubes, beside the one or two lines which seemed peculiar to themselves, gave others which were proved by direct comparison, to coincide with those of carbon and hydrogen, though the relative intensities seemed somewhat altered.

Supposed coincidences have been pointed out by Angström, Vogel, and others, between the auroral spectrum and those of the various gases, such as oxygen, nitrogen, and hydrogen, which are present in the atmosphere. Unfortunately these coincidences do not extend to the one bright line which has been accurately measured, but only to the fainter ones, the positions of which are so doubtful that they might be made to correspond with any spectrum the lines of which were tolerably numerous, so that, intrinsically probable as they may be, we cannot regard them as positively established.

Absolutely no coincidence has been made out between the bright yellow-green line of the aurora and a principal one of any other known spectrum, and the same may be said of the sharp red line which occasionally flashes out in the spectrum, of red auroræ. Mr. Capron, however, points out that the green line coincides with a faint atmospheric absorption band, while the red line seems to occupy the position of the well-known *a* line of the solar spectrum, which Prof. Smyth has shown to be due to dry air.

It would not be fair to conclude our notice of "Auroræ" without a few words of praise to the admirable illustrations, several of which are chromolithographs. Of these perhaps the best in artistic effect is a facsimile of a water-colour drawing of a white aurora seen by the author at Kyle Akin in Skye. But in fact the whole appearance of the book suggests at first glance art rather than science, and we should suppose it is but rarely that a purely scientific treatise has appeared in so ornamental a dress.

OUR BOOK SHELF

A Treatise on Metalliferous Mines and Mining. By D. C. Davies, F.G.S. 8vo. (London: Crosby Lockwood and Co., 1880.)

THE objects of this book, as stated in the preface, are "to describe in a concise and systematic manner the conditions under which metallic ores are found in different countries in the world," and further, "by defining the zones occupied by the various metallic ores to lessen somewhat the amount of unsuccessful search for them." For the first purpose the author notices a large number of mines in various parts of the world, partly from his own observations and partly from accounts published in special journals and in the transactions of scientific societies; and for the second, he deduces from such descriptive matter certain general conclusions, which, in their more important points, are as follows:—

"Gold and silver never occur in strata newer than the carboniferous period."

"Copper ores with trifling exceptions are only found in the lower Cambrian carboniferous and new red sandstone formations."

"The highest horizon of lead ores is in the carboniferous limestone."

The conclusions are apparently derived from the study of phenomena in Wales, and to render them universally applicable all that is necessary is to reconstruct the geology of the rest of the world to suit them, which the author does in a thorough-going fashion. Thus the system requires for Cornwall that the age of the granites should be Laurentian, and the killas and other schistose rocks Cambrian, Silurian, Devonian, &c., in regular succession; and therefore the author concludes that the received view which makes the granite post-carboniferous is a mistake, and corrects his authorities accordingly, even when quoting their observations. Thus in reproducing Dr. Foster's account of the Hay Tor iron ores he disputes their probable carboniferous age, and states that they may belong to an older group, and that possibly of a still older age are the deposits of the West of Ireland, which are found interstratified on the basaltic and porphyritic rocks that skirt the west coast. It appears from a preceding page that by these are meant the iron ores of Antrim, which occur in miocene basalts on the north-east coast between Larne and the Giant's Causeway, and about whose age no question can possibly be raised by any one with the smallest geological knowledge.

Much of the information concerning foreign mines is exceedingly inaccurate, indeed it is difficult to see whence some of it is derived. For example, on p. 240, in a paragraph describing the zinc ores of Silesia, it is stated that the calamine of that country averages 20 to 30 per cent. of metallic zinc, which by selection and dressing is brought up to 70 per cent.; that in 1876 sixty-four mines produced 31,315 tons of zinc ore, and a reference to a paper by Huene in the *Journal* of the German Geological Society is given as an authority. As these statements are contrary to what is generally known upon these subjects, an attempt has been made to verify them; and it appears that (1) the average yield of the Silesian zinc ores in 1876 as smelted was 11·84 per cent.; (2) the production of zinc ores in Silesia in 1876 was 449,374 tons; (3) the paper by Huene, published in 1851, has nothing whatever to do with Silesia, as it describes some zinc mines at Bergisch-Gladbach near Cologne.

The above examples taken quite at random will be sufficient to show the generally untrustworthy character of the book. H. B.

LETTERS TO THE EDITOR

[*The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.*

[*The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.*]

Why the Air at the Equator is not Hotter in January than in July

THE following, I think, is the explanation of Mr. Fisher's difficulty (NATURE, vol. xx. p. 577), why the January tempera- at the equator when the earth is in perihelion is not much higher than in July when in aphelion. The temperature to which Mr. Fisher refers is the ordinary temperature as indicated by the shade thermometer, which of course is simply that of the air. The difficulty is more apparent than real, for if we examine the *indirect* results which follow from the present distribution of land

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and water, we shall see that there is no reason whatever why the air at the equator should be hotter in January than in July.

It is well known that, notwithstanding the nearness of the sun in January, the influence of the present distribution of land and water is sufficient to make the mean temperature of the whole earth, or, what is the same, the mean temperature of the air over the surface of the earth higher in July than in January. The reason of this is obvious. Nearly all the land is in the northern hemisphere, while the southern hemisphere is for the most part water. The surface of the northern or land-hemisphere, for reasons to which I need not here refer, becomes heated in summer and cooled in winter to a far greater extent than the surface of the southern or water hemisphere. Consequently when we add the July or midsummer temperature of the northern to the July temperature of the southern hemisphere, we must get a higher number than when we add the January or midwinter temperature of the former to the January temperature of the latter. For example, the mean July temperature of the northern hemisphere, according to Dove ("Distribution of Heat on the Surface of the Globe") is 70°·9, and that of the southern hemisphere 53°·6; add the two together and we have 124°·5, which gives a mean for both hemispheres of 62°·3. The mean January temperature of the northern hemisphere is 48°·9, which, added to 59°·5, the mean January temperature of the southern hemisphere, gives only 108°·4, or a mean of 54°·2. Consequently the air over the surface of the globe is hotter in July by 8° than in January, notwithstanding the effects of eccentricity. It is obvious that, were it not for the counteracting effects of eccentricity, the difference would be much greater. Ten thousand years ago, when eccentricity and the distribution of land and water combined to produce the same effect, the difference must have been far greater than 8°.

But it will be asked, How can this affect the air over the equator, which is not situated more on the one hemisphere than on the other? It is true that those causes have but little *direct* effect on the air at the equator, but *indirectly* they have a very powerful influence. The air is continually flowing in to the equatorial regions from both hemispheres. In fact, the air which we find there is derived entirely from the temperate regions. In July we have the northern trades coming from a hemisphere with a mean temperature as high as 70°·9, and the southern trades coming from a hemisphere with a mean temperature not under 53°, while in January the former trades flow from a hemisphere as low as 50°, and the latter from a hemisphere no higher than 60°. Consequently the air which the equatorial regions received from the trades must have a higher temperature in July than in January. The northern is the dominant hemisphere; it pours in hot air in July and cold air in January, and this effect is not counterbalanced by the air from the opposite hemisphere. The mean temperature of the air passing into the equatorial regions ought therefore to be much higher in July than in January, and this it no doubt would be were it not, let it be observed, for the counteracting effects of eccentricity. The tendency of the present distribution of land and water, when our northern winter occurs in perihelion, is to counteract the effects of eccentricity. But ten thousand years ago, when our winters were in aphelion, that cause would co-operate to intensify the effects of eccentricity. In fact, it would actually more than double the effects then produced by eccentricity. Now if the influence of the present distribution of land and water is so great as not merely to counteract but to reverse the effects of eccentricity to the extent of making the mean temperature of the earth 8° warmer in July than in January, it is not surprising that it should be sufficient to make the equatorial regions at least as warm in the former as in the latter period.

The fact that the equator at present is not hotter when the earth is in perihelion, instead of being an objection to the theory that the glacial period was due to an increase of eccentricity, as Mr. Fisher supposes, is in reality another strong argument in its favour, for it shows that a much less amount of eccentricity would suffice to induce a commencement of glacial conditions in the northern hemisphere than would otherwise be required, were it not for the circumstance to which Mr. Fisher refers. This objection, like many others which have been urged against the theory, arises from looking too exclusively at the *direct* effects of eccentricity.

There is another cause which must also tend to lower the January and raise the July temperature of the equator, viz., the northern trades pass further south in January than in July, and consequently cool the equatorial regions more during the former than the latter season. This general tendency of the trades to lower the temperature of the equatorial regions more in January than in July is of course subject to modifications from the monsoons, the rainy seasons, and other local causes; nevertheless, so long as the present distribution of land and water endures, so long will eccentricity have a counteracting effect upon the temperature of the air at the equator, which but for that would be hotter in July than in January.

Mr. Fisher somewhat misapprehends what he designates my "fundamental proposition." What I stated was "the temperature of a place *other things being equal* is proportional to the heat received from the sun." Those who have read what I have written on this point will remember that what I mean is, that if the temperature of any place depended alone on the direct heat of the sun that temperature would be proportional to the amount received. But then there is no such spot on the face of the globe—there is no place where heat or cold distributed by ocean or aërial currents does not affect the temperature—and I have in "Climate and Time," pp. 41–44, proved that, with the exception of the Arctic regions, there is no part where the temperature is so much affected by those currents as the equator. Were it not for the cooling effect produced by them the equator would be uninhabitable. No knowledge whatever as to the intensity of the sun's heat can be obtained from observations on the temperature of the air at the equator. The comparatively cold air flowing in from the temperate regions has not time to be fully heated by the sun's rays before it rises as an ascending current and returns to the temperate regions from whence it came. More than this these trades prevent us from being able to determine with accuracy the intensity of the sun's heat from the temperature of the ground; for the surface of the ground in equatorial regions is kept at a much lower temperature by the air blowing over it than is due to the intensity of the sun's heat. It thus becomes a very intricate problem to determine how much the surface of the ground is kept below the maximum temperature by the heat absorbed by the moving air.

I may add that although my estimates of the lowering effect resulting from the decrease of the sun's heat arising from increase of distance were computed according to Newton's law, yet I distinctly stated that this law holds only approximately true, but that nevertheless, for reasons given at p. 34 of "Climate and Time," it would be found near enough for my purpose.

JAMES CROLL

A Possible Consequence of our Present Weather

I HAVE observed on several occasions that abnormally cold weather in November has been followed by an unusually mild mid-winter and January. These may possibly have been mere accidental coincidences, or they may be connected by a link of causation thus. Our climate, and more especially our winter climate, is largely influenced by the Gulf Stream, and whatever augments this raises our winter temperature, and *vice versâ*.

How, then, is the Gulf Stream likely to be affected by an unusual prevalence of Arctic winds and unusual cold in these latitudes? Such winds, must, to some extent, drive the waters of the Atlantic towards the source of the Gulf Stream, and tend to heap them there, and if there is any truth in the theory which attributes ocean currents to differences of oceanic temperatures, the present unusually cooled waters of the temperate zone will co-operate with the winds and augment this accumulation by their underflow. I do not mean that these combined actions are reversing the Gulf Stream at the present time, but simply that they are exerting a counter action or retarding influence which must result in augmenting the normal magnitude of the reservoir, or tropical accumulation, the outflow of which constitutes the Gulf Stream, and that thus the volume and velocity of the tropical waters which usually flow towards our coast will be augmented when the pressure of the present Arctic winds shall cease, and that our climate will be influenced accordingly. If I am right we may, in spite of present symptoms, or rather on account of them, have an unusually warm Christmas season and January.

This idea is not thrown out as a mere weather prophecy, but as a suggestive hypothesis and an incentive to what appears to me to be a very important and a much neglected branch of meteorological research, viz., systematic observation and record of the variations of the Gulf Stream. The countries whose coast is washed by this beneficent river of ocean are deeply interested in its movements. The Norwegians have already done something towards recording its variations, but so far as I can learn we, who are almost as deeply concerned as they are, have done little or nothing

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